

HOMING FACULTY OF HYMENOPTERA

IN connection with Sir John Lubbock's paper at the British Association, in which this subject is treated, it is perhaps worth while to describe some experiments which I made last year. The question to be answered is whether bees find their way home merely by their knowledge of landmarks or by means of some mysterious faculty usually termed a sense of direction. The ordinary impression appears to have been that they do so in virtue of some such sense, and are therefore independent of any special knowledge of the district in which they may be suddenly liberated; and, as Sir John Lubbock observes, this impression was corroborated by the experiments of M. Fabre. The conclusions drawn from these experiments, however, appeared to me, as they appeared to Sir John, unwarranted by the facts; and therefore, like him, I repeated them with certain variations. In the result I satisfied myself that the bees depend entirely upon their special knowledge of district or land-marks, and it is because my experiments thus fully corroborate those which were made by Sir John that it now occurs to me to publish them.

The house where I conducted the observations is situated several hundred yards from the coast, with flower gardens on each side and lawns between the house and the sea. Therefore bees starting from the house would find their honey on either side of it, while the lawns in front would be rarely or never visited—being themselves barren of honey and leading only to the sea. Such being the geographical conditions, I placed a hive of bees in one of the front rooms on the basement of the house. When the bees became thoroughly well acquainted with their new quarters by flying in and out of the open window for a fortnight, I began the experiments. The *modus operandi* consisted in closing the window after dark when all the bees were in their hive, and also slipping a glass shutter in front of the hive door, so that all the bees were doubly imprisoned. Next morning I slightly raised the glass shutter, thus enabling any desired number of bees to escape. When the desired number had escaped, the glass shutter was again closed, and all the liberated bees were caught as they buzzed about the inside of the shut window. These bees were then counted into a box, the window of the room opened, and a card well smeared over with birdlime placed upon the threshold of the beehive, or just in front of the closed glass shutter. The object of all these arrangements was to obviate the necessity of marking the bees, and so to enable me not merely to experiment with ease upon any number of individuals that I might desire, but also to feel confident that no one individual could return to the hive unnoticed. For whenever a bee returned it was certain to become entangled in the birdlime, and whenever I found a bee so entangled, I was certain that it was one which I had taken from the hive, as there were no other hives in the neighbourhood.

Such being the method, I began by taking a score of bees in the box out to sea, where there could be no landmarks to guide the insects home. Had any of these insects returned, I should next have taken another score out to sea (after an interval of several days, so as to be sure that the first lot had become permanently lost), and then, before liberating them, have rotated the box in a sling for a considerable time, in order to see whether this would have confused their sense of direction. But, as none of the bees returned after the first experiment, it was clearly needless to proceed to the second. Accordingly I liberated the next lot of bees on the sea-shore, and, as none of these returned, I liberated another lot on the lawn between the shore and the house. I was somewhat surprised to find that neither did any of these return, although the distance from the lawn to the hive was not above 200 yards. Lastly, I liberated bees in different

parts of the flower garden, and these I always found stuck upon the birdlime within a few minutes of their liberation. Indeed, they often arrived before I had had time to run from the place where I had liberated them to the hive. Now, as the garden was a large one, many of these bees had to fly a greater distance, in order to reach the hive, than was the case with their lost sisters upon the lawn, and therefore I could have no doubt that their uniform success in finding their way home so immediately was due to their special knowledge of the flower garden, and not to any general sense of direction.

I may add that, while in Germany a few weeks ago, I tried on several species of ant the same experiments as Sir John Lubbock describes in his paper as having been tried by him upon English species, and here also I obtained identical results: in all cases the ants were hopelessly lost if liberated more than a moderate distance from their nest.

GEORGE J. ROMANES

THE HEIGHTS OF CLOUDS

FROM the Upsala Observatory comes an account of fairly exact measurements of the heights of clouds during the summer of last year, and a very interesting publication it is. It appears that when the circumpolar expeditions were planned the Swedish Meteorological Observatory furnished their station at Spitzbergen with three theodolites, of a somewhat novel though simple construction, for the double purpose of observing the altitude of the aurora and that of clouds. The difficulty that has always been felt in such observations has been that of easy intercommunication between the different observers, so as to fix on the particular part of the cloud of which the height was to be measured. Thanks to modern invention this difficulty was got over by connecting each station with a telephone. The reported good results obtained at the circumpolar station—the publication of which, by the by, has not been done as yet—induced Herr Hildebrandsson, the director of the meteorological observatory at Upsala, to commence a set of similar observations there. On a couple of pillars, about 450 yards apart, and placed on an approximately north and south line, a couple of theodolites were erected, the stations being connected by telephones. The theodolites employed may be described as ordinary theodolites, the object glass of the telescope being replaced by a large open ring, across which were stretched a couple of cross wires, whilst the eye-piece consisted of a simple hole of 3mm. in diameter. When observing near the sun dark glasses would be placed in front of this orifice. As might be expected, there are several unavoidable errors in using these instruments, the principal of which are the uncertainty of an identical point in a cloud being measured at each station, and the want of synchronism of the observation—a very important point when clouds are travelling with any speed. The method of observation was somewhat laborious, and was as follows. The two observers, each at a theodolite, agreed as well as they could on the point in the cloud to be observed, and at a particular time, fixed upon in advance, brought the cross wires on this somewhat indefinite spot, and then read their instruments, noted the time of observation, described the cloud, and if possible sketched it. A second observation of the same point gave the direction and rate of motion of the cloud. Perhaps one of the most easily observed clouds is the cumulus, and we find from a table given that the probable error of observation is very considerable. Thus, in one whose height was calculated to be 1,639 metres, the probable error of one observation was 748 metres, and of the mean of 16 observations 187. Out of 101 observations the mean height of a cumulus was 1,690 metres, and the probable error of the mean 40

“Mesures des Hauteurs et des Mouvements des Nuages.” Par N. Ekholm et K. L. Hagström.

metres. The labour to attain even such accuracy is very great. The surprise is that at Upsala they did not adopt a photographic theodolite such as is now, we believe, in daily use at Kew. In the Kew "nephographs," as they are called, the telescope is replaced by a camera, and the observations do not involve half the labour of eye-observations. For instance, when the two nephographs are in a fixed position the manipulations are simplicity itself. One observer telephones to the other the cloud whose height it is desired to ascertain. By means of a very simple pointer both direct their cameras to the cloud, having inserted a dry plate in position. The lenses are closed by shutters, both of which can be opened and then closed with any desired rapidity by an electrical arrangement from one station. The exposures are thus made simultaneously, and the photograph must include every point in the cloud. The position of the cloud is fixed by crossed lines etched on a glass plate which is in contact with the dry plate, and which always occupies the same position, and from these cross lines, which are impressed on the two negatives, any desired point is measured. The readings of the graduated circles of the nephoscope having been taken the height and distance of the cloud is readily calculated. It might be supposed that considerable errors might be made even with this arrangement as the solid angular distance included is somewhere about 55° , and the objects within this are impressed on a plate less than six inches square. As a matter of fact, such is not the case. Measurements of objects a couple of miles off, and at known distances from the observer, have been observed with an error of less than 1 per cent., a base of 250 yards having been used—an accuracy which is far greater than could be obtained by eye-observations when the object to be observed is uncertain in outline, and when there is no definitely fixed point to observe. It must not, however, be supposed that there are no difficulties in photographing clouds of every description. It requires, for instance, a keen judgment to hit off the exposure necessary to differentiate between the white clouds in the higher regions the pale blue sky against which they are projected. All such difficulties are to be overcome with practice. It is to be hoped that before long the Upsala Observatory will adopt such a plan as we have indicated, when the results they obtain will be even more valuable and be less laboriously attained than they are at present.

The following table gives the height of the different characters of clouds at Upsala:—

Stratus	625 metres.
Nimbus (lower)	1,115 "
" higher	2,185 "
Cumulus and cumulo-stratus	top 1,690
	base 1,307
	mean 1,498
Lower alto-cumulus	1,988
Higher " "	4,242
Cirro-cumulus	5,513
Cirrus	6,823

The authors point out that, according to their observations, apparently there are seven levels, each one occupied by a different species of cloud, viz.: 600, 1,100, 1,500, 2,000, 42-4,600, 58-6,600, and 80-8,600 metres; and these levels agree with those deduced by M. Vettin of Berlin, who deduced them from a different mode of observation. There are several remarkable tables, some of which give the diurnal variation in the height of clouds, others the diurnal variation of the frequency of high clouds at Upsala during the summer, others again which discuss the question of the effect of the height of the barometer on the cloud masses. One of the most interesting sections of the memoir is that on the calculation of the velocity of wind at different heights from the movements of clouds.

On the whole, the Observatory at Upsala is to be congratulated on the step it has taken in making systematic

observations of cloud heights and velocities. It is a matter of capital importance to meteorology that such should be undertaken in various localities, not only at or near the sea level, but also at as high altitudes as possible. Were the cloud levels, for instance, the same at all places, mountainous districts would be very much more cloud bound than we know is the case. Observations of clouds in the Alps show that the levels at which the different classes are to be found exceed the heights which are shown in the table above; and it remains to ascertain not only the effect of barometric pressure on the levels, but also the disturbing effect caused by the elevations in the land. Such observations might well be added to the observatory at Ben Nevis, and no doubt some enthusiastic meteorologist would be willing to spend a summer in the Alps to make observations at a still higher station. Until work such as this is undertaken the subject can only be partially discussed on scientific grounds.

W. DE W. A.

THE RECENT TOTAL ECLIPSE OF THE SUN

WE have received the following communications:—

THE news that bad weather seriously interfered with the work of the Government Survey parties, sent to observe the eclipse of the 9th inst. from points on the centre line of totality, induces me to send you the accompanying incomplete sketch and hasty account by to-day's mail:—

I observed the eclipse from Tahoraite, the present southern terminus of the Napier-Wellington Railway, a point well within the belt of totality, but some forty miles north of the centre line.

I went, determined to concentrate my whole attention on the corona, and the corona alone—I did not even take my watch. My eclipse observations are therefore necessarily very incomplete.

After a stormy night (alternate showers of rain and hail, with a bitterly cold wind), day-dawn brought a clear sky; but a heavy bank of clouds far away to the south boded no good to observers in that direction. The cold was bitter, and fresh snow lay very low down on the neighbouring hills.

The first contact occurred not long after sunrise, the atmosphere in the east being rather hazy, and the light *pale* (other observers say *ruddy*). At first the temperature of the air seemed to rise steadily, but when the sun's disk was a quarter obscured, it began to fall again, and as totality approached the cold became severe.

When the occultation of the sun had reached three-quarters, the so-called "livid" character of the light became very marked, and about ten minutes before totality a curious and tremulous play of light on the ground—like dark ripples or moving "marblings," if I may use the word, became apparent.

In order to keep my eyes as sensitive as possible to the faint light of the corona when it should become visible, I only watched the sun (through a telescope) for a few minutes after first contact, I then averted my gaze, and fixed it persistently on the dark-green bush surrounding the Tahoraite clearing. All I noticed during my hasty survey of the disk was two small and one large spot, the latter close to the limb at about 90° (see sketch), and surrounded by faculæ.

The moment "totality" occurred I turned my gaze towards the sun, and having previously, to save time, drawn disks on several pages of my pocket-book, I hurriedly took sketch after sketch of the shape of the corona, the rays of which were much better marked than I had been led to expect. My object in taking several sketches was to record any change in the position of the rays. I took five during the short time of totality, and their agreement is so clear as regards the number and relative